$SnCl_2$. Again, silica (as has been shown by Mendelejeff long before Henry) certainly has a more complicated molecular formula than SiO_2, say $nSiO_2$, otherwise it would be a gas, analogous to CO_2, for $SiCl_4$ boils at 57° and CCl_4 at 76°. But would it be reasonable to say that the SiO_2 molecules are held together by the fifth valency of Si?

The only satisfactory answer to the above question is that in molecules R_2Cl_6 the two RCl_3 groups are held together by their own—let us say residual—affinity.

I think that the study of the question touched in the above lines will greatly add to the development of our theoretical ideas in chemistry, and deserves the careful attention of chemists.

B. BRAUNER.

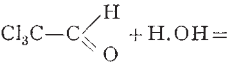
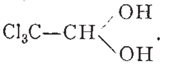
Bohemian University, Prague, January 8.

IN my letter, referred to by Dr. Brauner, I considered only the molecular formulæ of the chlorides, as indicated by the experimental results.

Dr. Brauner has now, in his interesting letter, enlarged the field of discussion by introducing the question of the constitutional formulæ of these compounds, involving the vexed question of the valency of the elements. Dr. Brauner points out the importance of the proof of the trivalency of these metals in their chlorides when in the gaseous state at high temperatures, and he then discusses the valency of the metals when their chlorides have the formula R_2Cl_6.

Although I am inclined to believe that the views expressed by Dr. Brauner may eventually prove to be correct, yet I cannot help thinking that we are not yet in a position to speak with certainty on one or two points. Dr. Brauner states that the number of "bonds" by which the atoms of the metal in the compounds R_2Cl_6 are united is certainly not *one*, and again, that Sn_2Cl_4 cannot be expressed by the formula $Cl_2Sn'''—Sn'''Cl_2$. But is this quite certain? Indium, the metal next to tin in the seventh horizontal series, is mono-, di-, and tri-valent in the compounds $InCl$, $InCl_2$, $InCl_3$; is it, then, quite unwarrantable to assume that tin is di-, tri-, and tetra-valent in the compounds $SnCl_2$, Sn_2Cl_4, $SnCl_4$?

As regards silica, $nSiO_2$, the properties of the compound seem to indicate that n is a large number, and here, unless we arrange the Si atoms in a ring, like the carbon atoms in benzene, we must probably look on the compound as "molecular."

It is difficult to find experimental evidence which bears directly on the question; perhaps such results as those obtained by Dr. Ramsay and myself on the dynamical and statical methods of measuring vapour-pressures may throw some light on the matter. We find that the two methods give identical results with ammonium chloride, nitrogen peroxide, and acetic acid, just as they do with all stable solids and liquids, but very different results with all the other dissociating substances examined. This appears to indicate some difference in the molecular arrangement of the two groups of compounds, but it is quite uncertain whether the difference is one between "molecular" and "atomic" compounds, or between compounds formed in a very simple manner, such as $2O_2N = O_2N—NO_2$, and those in which there is a breaking down of a stable molecule, as in the case of chloral hydrate,  $Cl_3C—C{<}^{H}_{O} + H.OH =$  $Cl_3C—CH{<}^{OH}_{OH}$.

I am at present engaged in a study of the vapour pressures of halogen compounds by both methods, and it is just possible that some further light may be thrown on this question by the results of the investigation.

SYDNEY YOUNG.

University College, Bristol, January 15.

Remarkable Rime and Mist.

YOU have a letter in NATURE of January 17 (p. 270), signed "Annie Ley," which induces me to add the following:—We had here on January 6 an extraordinary rime formed at a temperature varying between 21°·5 and 25°·7 (1° warmer on the grass than at 4 feet), the air being almost calm. This rime increased in thickness and in length with the height above the ground. The length measured of the deposit on a birch-tree at 5 feet was ⅝ of an inch; at 10 feet, 1 inch; at 15 feet, 1¼ inch; and at 25 feet, 1½ inch. The hoar was nearly horizontal, pointing downwards at an angle of 15°. That deposited on the grass, however, was perpendicular, rising with a thin stem and having a large funnel-shaped head. Suddenly, at 10 a.m. of the 7th (next morning), the whole of the rime (still frozen) fell to the ground, and under the birch-tree of 30 feet in height and 18 feet across (sparse of branches, and none for 10 feet), the fallen rime covered the ground to the depth of rather more than 2 inches, and this, when melted, yielded 0·550 of an inch of water (or 3¾ inches of rime to 1 of water). The rime on the grass when carefully collected and melted yielded 0·033 of an inch. There was a dense mist whilst the rime was being deposited.

From this elevation (530 feet) we frequently look over dense mists that cover the water of the Bristol Channel and see the hills of Somerset, Devon, Monmouth, and Glamorgan. On the 19th instant, with a hoar frost, over the Bristol Channel was a dense dark mist apparently extending 100 feet into the air. This mist, at 8.30 a.m., rapidly changed on its upper surface to cirri clouds, and then to transparent vapour, and in an hour the whole mist by this process had disappeared. Elsewhere the sky was cloudless. These mists of the Bristol Channel change on their upper surface sometimes to cirro-strati, sometimes to cumuli, and twice they have been known to change to thunder-clouds during the last two years. The change to cirri has only been seen once.

E. J. LOWE.

Shirenewton Hall, near Chepstow, January 22.

Cercyonis alope and *nephele*.

IN his review (NATURE, December 27, 1888, p. 193) of my work on butterflies now in course of publication (in which I receive a far more generous treatment than I am accustomed to), Captin Elwes thinks me illogical in holding to the probable specific distinction of *Cercyonis alope* and *nephele*, and at the same time the specific unity of the Eastern American forms of *Cyaniris pseudargiolus*, *lucia*, *violacea*, and *neglecta*; and suggests as to the former that climatic differences in the regions they respectively occupy may have brought about the distinctions noted.

On general grounds, it seems in the highest degree probable that climatic differences have had much to do with the origin of the different forms in both cases, be they species formed or forming. But surely Captain Elwes is confusing the judgment when he fails to make a distinction between the successive seasonal forms of a digoneutic butterfly, as in the case of *Cyaniris* and de Nicéville's Indian species to which he alludes, and the synchronous variation of a monogoneutic species, like those (or that) of *Cercyonis*.

SAMUEL H. SCUDDER.

Cambridge, U.S.A., January 10.

MODERN VIEWS OF ELECTRICITY.[1]

PART IV.—RADIATION.

XIII.

Possible Accounts of the Faraday and Hall Effects.

THE account I have given of the magnetic rotation of the plane of polarization has made it depend on the phenomenon of hysterĕsis, in a way which may be thus summarized. The value of μ for increasing magnetization is different from that for decreasing magnetization; an electric displacement such as occurs in every half-swing of a light-vibration is resolvable into two opposite circular components, one of which increases, while the other decreases, any magnetization already existing in the direction of the ray: the value of μ affects the speed of transmission of light; hence the two circular components will not proceed at the same pace, and the direction of vibration will infinitesimally rotate. The same thing is repeated at every half-swing, the elemental rotations being all in the same sense, and so the ultimate rotation of the plane of polarization in transparent bodies is accounted for.

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[1] Continued from p. 13.

The Hall effect observed in conductors follows at once; for the rotation of a displacement is equivalent to combining it with a small perpendicular displacement; and it is this perpendicular or transverse E.M.F. exerted by a magnetic field which Hall discovered. At the same time, there are one or two facts which militate against this view of the Hall effect, chief among which is the singular behaviour of nickel, which rotates light one way and electric displacement the other way. For some time it was possible to hope for a way out of this through the usual convenient avenue of "impurity"; but now that both experiments have been performed on the same identical piece of metal, still with opposite results, this exit is closed. In this unsettled state, so far as I know, the connection between the rotation of light and the Hall effect at present stands.

It may be well here to repeat the caution, appended as a footnote to the last article, not to assume that this account of the magnetic rotation of light and the Hall effect is true. If true, however, it is convenient as linking the phenomena on to hysterēsis, and the direction of the effect in iron is correctly given—namely, a rotation against the magnetizing current.

Prof. Ewing has since pointed out, in a letter to me, that, attending more precisely to the instruction of his curves, we find the difference in μ for positive and negative magnetizing forces only lasts through a number of cycles for the time during which the final state has been approached, and does not persist after a steady state has been reached. This would make the magnetic rotation of light a function of time; and certain experiments by Villari on spinning a glass disk between the poles of a magnet, so that fresh and fresh portions of glass were continually exposed to the magnetic field, showed a marked falling off in the amount of rotation as soon as high speeds were obtained; thus proving, apparently, that a certain short time was necessary to set up the effect. This experiment, and other modifications of it, want repeating, however.

Prof. Ewing has subsequently expressed a doubt as to whether the kinematic resolution of a displacement into two equal opposite circular components is, under the circumstances, legitimate.

Prof. Fitzgerald has further pointed out that, although when attending to one element only the theory might possibly work, yet, as soon as one takes into account the whole wave-front, it breaks down; for all the main magnetic disturbance lies in the wave-front, as is well known, and the extra magnetic disturbance which I have postulated as a consequence of electrostatic displacement is annulled by interference of adjacent elements.

If I were quite sure that there were no vestige of truth in the suggestion I have made, I should, of course, withdraw it; but, as I do not feel perfectly sure either way, I leave it in a dilapidated condition for the present.

Another and apparently distinct account of the magnetic rotation can also be hinted at, which links the phenomenon on to the facts of thermo-electricity. It labours under worse disadvantages than the preceding, being more hazy.

Referring back to NATURE, vol. xxxvii. p. 12, we find that, to explain what is called the "Thomson effect" in metals, we were led to suppose a connection between one kind of electricity and some kinds of matter more intimate than between the other kind of electricity and the same matter. Thus, the atoms of iron were said to have a better grip of positive electricity than of negative; while copper, on the other hand, had a better grip of negative than of positive. All metals could be arranged in one or other of the two classes, with the exception of lead, which appears to grip both equally. It is the same phenomenon as was originally named by Sir W. Thomson, "the specific heat of electricity in a substance." Certain it is that vibrating atoms of iron push positive electricity from the more rapid to the less rapid places of vibration—that is, from hot to cold—and a whole class of the metals do the same; while another class, like copper, push it from cold to hot.

Permitting ourselves to picture this effect as a direct consequence of the Ohm's law relation between electricity and matter, combined with a special relationship between certain kinds of matter and one or other kind of electricity, a relationship which can exhibit itself in other ways also, we get a possible though rather hazy notion of a Faraday rotation in a magnetic field by supposing that the Amperian molecular currents in these substances consist not of precisely equal positive and negative currents, but of opposite currents slightly unequal; say, for instance, that the density of the positive constituent of the bound ether of a substance is slightly different from that of the negative constituent, so that on the whole the bound ether in a magnetized molecule is slowly rotating one way or the other, at a pace equal to the resultant rotation of its constituents. Suppose that in iron the positive Amperian electric current is the weaker of the two, then the ether, as a whole, will be rotating with the negative current, and accordingly an ethereal vibration entering such a medium will begin to screw itself round in a direction opposite to that of the magnetizing current. Whereas in copper or other such substance it would be rotated the other way.

According to this (admittedly indistinct) view, lead ought to show no rotatory effect at all; and of course, therefore, no Hall effect either. And the classes into which metals are divided by the sign of their Hall effect should coincide with the classes into which the sign of their Thomson effect throws them.

Hall finds that, of the metals he examined, iron, cobalt, and zinc fall into one class, while gold, silver, tin, copper, brass, platinum, nickel, aluminium, and magnesium fall into the other. Now, referring to the thermo-electric results of Prof. Tait, we find iron, cobalt, platinum, and magnesium with a negative sign to their Thomson-effect-coefficient, or with lines in the thermo-electric diagram sloping downwards; while gold, silver, tin, copper, aluminium, and zinc slope upwards, or have a positive sign to their "specific heat of electricity."

According to this, therefore, the discordant metals are zinc, platinum, and magnesium. The proper thing to say under these circumstances is that the metals used in the very different experiments were not pure. They certainly were not; but I do not feel able to conscientiously bolster up so inadequate a theory by help of this convenient fact.

In the *Philosophical Magazine* for May 1885, Mr. Hall gives some more measurements, showing that in bismuth the effect is enormous, and in the same direction as in copper, whereas in antimony it is also great, and in the same direction as in iron. All these things seem to point to some thermo-electric connection—whether it be of the sort I have vaguely tried to indicate, or some other.

Other Outstanding Problems.

Outstanding problems bristle all over the subject, and if I pick out any for special mention it will only be because I happen to have made some experiments in their direction myself, or otherwise have had my thoughts directed to them, and because they have not been so directly called attention to in the body of the articles.

Referring back to the end of Part II., "a current regarded as a moving charge," it is natural to ask, Is this motion to be absolute, or relative to the ether only, or must it be relative to the indicating magnetometer? In other words, if a charged body and a magnetic needle are flying through space together, as, for instance, by reason of the orbital motion of the earth, will the needle experience any deflecting couple?

It is one of many problems connected with the ether and its motion near gross matter—problems which the experiments of Fizeau (showing that a variable part of ether was bound with matter and transmitted with it, while another constant portion was free and blew through it) began to throw light upon ; aberration problems such as have been partially solved by the genius of Stokes ; problems connected with the motion of ether near great masses of matter, like those which Michelson is so skilfully attacking experimentally : it is among these that we must probably relegate the question whether absolute or relative motion of electric charges is concerned in the production of magnetic field, and what absolute motion through the ether precisely means. It is doubtless a question capable of being attacked experimentally, but the experiments will be very difficult. I believe that Prof. Ayrton has attempted them.

Referring back to Parts I., II., and III., we find a number of questions regarding momentum left unsettled. Has an electric current any true momentum mechanically discoverable? Now, this question, before it can be answered in the negative, will have to be attacked under a great number of subdivisions. One may classify them thus. Two main heads : (1) When steady, Does a magnet behave in the least like a gyrostat? (2) When variable, Is there a slight mechanical kick on starting or stopping a current? With four or more subsidiary heads under each, viz. (*a*) in metallic conductors ; (*b*) in electrolytes ; (*c*) in gases ; (*d*) in dielectrics.

Suppose the answer turns out negative in metals, it by no means follows that it should be negative in electrolytes too. In fact, as matter travels with the current in the case of electrolytic conduction, it is hardly possible that there is not some momentum, though it may be too small to observe—either a kick of the vessel as a whole at starting and stopping, or a continuous impact on an electrode receiving a deposit. The present writer has looked for these things, but after gradually eliminating a number of spurious effects the result has been so far negative. In a light quill vessel fixed to the end of a torsion arm, the main disturbance was due to variations of temperature which gradually introduced a minute air-bubble, and by kicking this backwards and forwards simulated the effects sought. In the case of the suspended electrode, convection currents in the electrolyte, caused by extra concentration or the reverse, seem determined to mask any possible effect.

One obvious though very troublesome source of disturbance in all cases is the direct effect of terrestrial magnetism on the circuit. To get over this, the writer not only made his circuits as nearly as possible of zero area, but also inclosed them in the iron case of a Thomson marine galvanometer, lent for the purpose by Dr. Muirhead.

In gases, the experiment of Mr. Crookes, where a stream of particles propels a mill inside a vacuum-tube —perhaps even the ancient experiment of the blast from a point—shows that momentum is by no means absent from an electric current through a gas.

To see if there are any momentum effects accompanying variation of electric displacement in dielectrics, the writer has suspended a mica-disk condenser at the end of a torsion arm, and arranged it so that it could be charged and discharged *in situ*. Many spurious effects, but no really trustworthy ones, were observed.

In the writer's opinion the subject is by no means thoroughly explored, and he only mentions his old attempts as a possible guide to future experimenters.

Then, again, there is the influence of light on conductivity. Annealed selenium, and perhaps a few other things, improve in conductivity enormously when illuminated. The cause of this is unknown at present, and whether it is a general property of matter, possessed by metals and other bodies to a slight degree, is uncertain ; for the experiments of Börnstein with an affirmative result for the case of metals have been seriously criticized.

Even though metals show no effect, yet electrolytes might possibly do so, but the effect, if any, is small ; and it is particularly difficult in their case to distinguish any direct radiation effect from the similar effect of mere absorbed radiation or heat.

The writer has found that a glass test-tube kept immersed in boiling water conducts distinctly better when the blinds of a room are raised than when they are lowered, though nothing but diffuse daylight falls upon it. But as the effect could have been produced by a rise in temperature of about the tenth of a degree, and as the absorption of diffuse daylight is competent to produce a rise of temperature as great as this in the glass of a thermometer-bulb even though immersed in boiling water, he feels constrained to regard the result, though very clear and distinct, as after all a negative one, and has accordingly not published it.

A few months ago I should have put in a prominent position among outstanding problems the production of electric radiation of moderate wave-length, and the performance with this radiation of all the ordinary optical experiments—reflection, refraction, interference, diffraction, polarization, magnetic rotation, and the like. But a great part of this has now been done, and so these things come to be now mentioned under a different heading.

Conclusion.

"Conclusion" is an absurd word to write at the present time, when the whole subject is astir with life, and when every month seems to bring out some fresh aspect, to develop more clearly some already glimpsed truth. The only proper conclusion to a book dealing with electricity at the present time is to herald the advent of the very latest discoveries, and to prepare the minds of readers for more.

Referring back to Part IV., vol. xxxviii. p. 418, we spoke confidently of a radiation being excited by electric oscillations, a radiation which travelled at the same rate as light, which is reflected and refracted according to the same laws, and which, in fact, is identical with the radiation able to affect our retina, except in the one matter of wave-length. Such a radiation has now been definitely obtained and examined by Dr. Hertz, of Karlsruhe, and in the last month of last year, Prof. von Helmholtz communicated to the Physical Society of Berlin an account of Dr. Hertz's latest researches. The step in advance which has enabled Dr. Hertz to do easily that which others have long wished to do, has been the invention of a suitable receiver. Light when it falls on a conductor excites first electric currents and then heat. The secondary minute effect was what we had thought of looking for, but Dr. Hertz has boldly taken the bull by the horns, looked for the direct electric effect, and found it manifesting itself in the beautifully simple form of microscopic sparks.

He takes a brass cylinder, some inch or two in diameter, and a foot or so long, divided into two halves with a small sparking interval between, and by connecting the halves to the terminals of a small coil, every spark of the coil causes the charge in the cylinder to surge to and fro about five hundred million times a second, and disturb the ether in a manner precisely equivalent to a diverging beam of plane-polarized light with waves about twice the length of the cylinder.

The radiation, so emitted, can be reflected by plane conducting surfaces, and it can be concentrated by metallic parabolic mirrors ; the mirror ordinarily used being a large parabolic cylinder of sheet zinc, with the electric oscillator situate along its focal line. By this means the effect of the wave could be felt at a fair distance, the receiver consisting of a synchronized conductor with a microscopic spark-gap, across which the secondary induced sparks were watched for. By using a second mirror like the first to catch the parallel rays and reconverge them to a focus, the effect could be appreciated at a distance of 20 yards. If the receiving mirror were rotated through a right angle, it lost its converging power on this particular light.

Apertures in a series of interposed screens proved that the radiations travelled in straight lines (roughly speaking, of course).

A gridiron of metallic wires is transparent to the waves when arranged with the length perpendicular to the electric oscillations, but it reflects them when rotated through a right angle, so that the oscillations take place along the conducting wires; thus representing a kind of analyzer proving the existence of polarized light. The receiver itself acted as analyzer, however, for if rotated much it failed to feel the disturbance.

Conducting sheets, even thin ones, were very opaque to the electrical radiation; but non-conducting obstacles, even such as dry wood, interrupt it very little, and Dr. Hertz remarks, "not without wonder," that the door separating the room containing the source of radiation from that containing the detecting receiver might be shut without intercepting the communication. The secondary sparks were still observed.

But the most crucial test yet applied is that of refraction. A great prism of pitch was made, its faces more than a yard square, and its refracting angle about 30°. This being interposed in the path of the electric rays, they were lost to the receiver until it was shifted considerably. Adjusting it till its sparks were again at a maximum, it was found that the rays had been bent by the pitch prism, when set symmetrically, some 22° out of their original course, and hence that the pitch had an index of refraction for these 2-foot waves about 1·7.

These are great experiments. As I write, they are but a month old, and they are manifestly only a beginning. They are very simple: I have repeated some of them already. They seem likely to settle many doubtful points. There has been a long-standing controversy in optics, nearly as old as the century, as to whether the direction of the vibrations was in, or was perpendicular to, the plane of polarization; in other words, whether it was the elasticity or the density of the ether which varied in dense media; or, in Maxwell's theory, whether it was the electro-magnetic or the electrostatic disturbance that coincided with that plane. This point has indeed by the exertion of extraordinary power been almost settled already, through the consideration of common optical experiments; but now that we are able electrically to produce radiation with a full knowledge of what we are doing, of its directions of vibration and all about it, the complete solution of this and of many another recondite optical problem may be expected during the next decade to drop simply and easily into our hands.

We have now a real undulatory theory of light, no longer based on analogy with sound, and its inception and early development are among the most tremendous of the many achievements of the latter half of the nineteenth century.

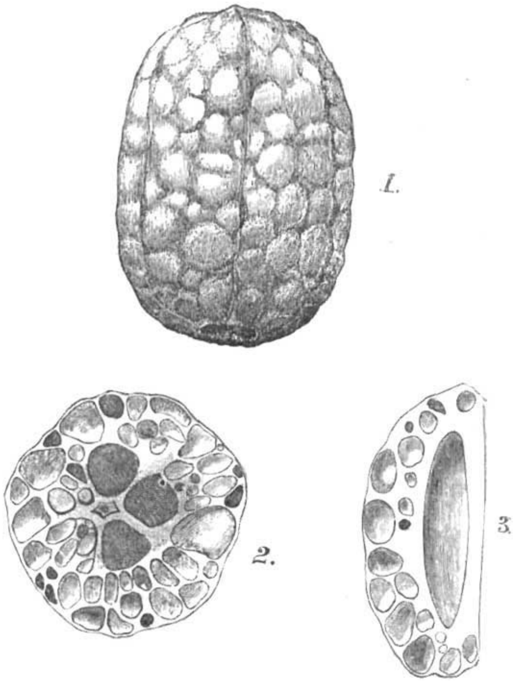
In 1865, Maxwell stated his theory of light. Before the close of 1888 it is utterly and completely verified. Its full development is only a question of time, and labour, and skill. The whole domain of Optics is now annexed to Electricity, which has thus become an imperial science.

OLIVER J. LODGE.

A JAMAICA DRIFT-FRUIT.

IN 1884, I made a collection of drift-seeds and seed-vessels washed ashore on the spit of land inclosing Kingston Harbour, Jamaica, called the Palisadoes. The collection was sent to Kew, and it was utilized by Mr. Hemsley in the appendix to the "Botany of the *Challenger* Expedition," vol. i. pp. 277-304, when discussing the oceanic dispersal of plants. In the collection there was a fruit I had not seen before, and which was equally new to Mr. Hemsley and the other officers at Kew. There were several specimens of this fruit obtained, and these were afterwards placed in the Kew Museums amongst other drift-fruit, to wait until sufficient material was forthcoming to lead to their identification.

The general character of the drift-fruits collected at Jamaica points to the conclusion that they were brought by the Gulf Stream current from the mouths of the Amazon and Orinoco. Jamaica lies directly in this current, and for a long period fruits unknown in the island itself have been collected on its shores. The most striking of these is the fruit of *Manicaria saccifera*, a palm confined to Trinidad and the mainland of South America. Sloane ("Natural History of Jamaica," vol. ii. p. 186) noticed this fruit more than two hundred years ago, and states that "it is frequently cast up by the waves on the shores of this island, and it is one of those fruits thrown (also) on the north-west islands of Scotland by the currents and seas." None of these fruits appear to have germinated in Jamaica, for the palm, which is a most striking object, with entire leaves, could not fail to be noticed if it existed there. The fruits are known at Jamaica as *sea-cocoanuts*, while at Barbados they are called *sea-apples*. Other South American fruits found drifted at Jamaica were *Carapa guianensis*, *Dimorphandra mora*, and *Astrocaryum* sp. The collection of drift-fruits, although by no means a new subject, is one which has not received the attention it deserves. Since the publication of the "Botany of the *Challenger*," the active part played by the sea in the dispersion of plants has been more fully recognized. Mr. Hemsley has given a summary of results, in which many interesting facts have been brought to light, and doubtless observers in numerous parts of the world are now engaged in making observations.



Representation of a Jamaica drift-fruit (natural size). 1, external aspect; 2, cross-section; 3, longitudinal section.